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Atmospheric Measurement Techniques Discussions

Interactive comment on "On the ability of pseudo-operational ground-based light detection and ranging (LIDAR) sensors to determine boundary-layer structure: intercomparison and comparison with in-situ radiosounding" *by* C. Milroy et al.

Anonymous Referee #1

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General comments:

The manuscript by Milroy et al. reports the outcome of an intercomparison of the boundary layer height retrieved using the algorithm by Martucci et al (2010) applied to the observations provided by a lidar and two ceilometers. Moreover, a comparison with the retrieval of the boundary layer height obtained using temperature inversions observed by in-situ co-located radiosounding measurements is proposed. The inter-

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comparison between the three sensors, presented in section 5.1, should be rather considered as an evaluation of the performances of the two ceilometers respect to the lidar. Lidar capability to retrieve boundary layer height has been largely investigated. ASL3000 higher performances, mentioned several times in the manuscript, are obviously related to the basic differences existing between an elastic backscatter lidar system and a ceilometer, that are related to the basic concept of the two sensors, designed for different purposes. The differences mainly concerns the laser source, sampling rate of the atmosphere and optical receiver (e.g. Dupont et al., 1994 - Bound. Layer Meteor; Matthias et al., 2002 – Atmos. Research).

My opinion is that the authors should address several aspects before providing a quantitative assessment of ceilometers vs. lidar performance in retrieving boundary layer (BL) height. This manuscript sounds more like a technical report of an intercomparison campaign with not strong advances in the scientific knowledge. Also the attempt to assess THT algorithm performances on the selected cases should be performed on a larger dataset that refers to a larger set of atmospheric scenarios. However, if one of the aim of manuscript is to assess THT performances, this should be explicitly mentioned in the paper. Moreover, a brief description of the error on the BL height retrieved using the THT algorithm should be reported. Moreover, their conclusions come out from a measurement dataset that is not sufficient for a significant statistical analysis.

I also encourage the authors to reconsider the section 5.2 where a comparison with in-situ measurements is described. The comparison is based on a restricted dataset that is not sufficient to perform a quantitative assessment of the reliability of both the definition of BL, based on the aerosol distribution, and the instrumental performances in detecting the BL. The statistics reported in this section aims at assessing the difference between the two methods for the retrieval of the boundary layer height. This difference are within a few hundreds of meters. This differences are consistent with the results presented by Seidel et al. 2010 (JGR), where several methods reported in literature for retrieving the boundary layer height from radiosounding measurements are compared

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over 10 years of data. Considering that a comparison between different instruments and different methods for the retrieval of the BL height is proposed in the manuscript, I suggest the authors to investigate larger dataset of co-located measurements before assessing the adequacy or inadequacy of the BL retrieval obtained using aerosol lidar measurements.

Specific comments:

1. Section 5.4 is very short and it is not necessary to let it separated from 5.3. I suggest to put together 5.3 and 5.4 sections.

2. The authors should be aware that they are dealing with different sensors, with different features, as different overlap functions, temporal sampling, laser sources, and soon. This means that an assessment of the performances of a technique or of an algorithm in retrieving the PBL should be assessed also in terms of different aerosol optical thickness scenarios before providing final conclusions that could be incomplete and wrong.

3. page 569, line 10: the ASL300 is an elastic backscatter lidar; this means that it is not able to provide profiles of the extinction coefficient without making a few strong assumptions. Please specify what assumptions are made in the retrieval of the extinction and backscatter coefficient.

4. page 569, line 25: in order to avoid confusion, please specify that the CHM15k laser source is diode-pumped Nd-YAG laser (1064 nm) yielding about 8 μJ per pulse at 5-7 KHz repetition rate. Moreover, additional information about differences in the divergence of the systems could be useful. As a whole, please stay consistent in the description of the three systems giving the same information for all of them.

5. page 570, line 3: in the eq (1), please mention that the additional term B is the background noise. Moreover, the authors mention the optical efficiency of the system using the term O(h). What about the quantum efficiency?

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6. page 570, line 22: please include a reference for supporting this approximation.

7. page 572, line 10-13: the following sentence is quite unclear “The intercomparison’s output in terms of SML and DRCL detections retrieved by the THT algorithm can vary signiïňĄcantly from case to case depending on meteorological conditions and on the diïňĂerent instrumental skills “. Please explain better.

8. page 572, line 18; please specify that X vs Y indicates you are referring to a couple of sensors.

9. page 574, line 2-4: please include a reference for supporting the BL retrieval method from the inversions in the radiosounding temperature profile. For example, I suggest Seidel et al., 2010.

10. page 574, line 22-25: this sentence again shows that your study should be considered as an assessment of ceilometer vs lidar performance in retrieving BL height and not as an intercomparison between three sensors. This should be explicitly mentioned throughout the paper.

11. page 576 line 3-4: the authors could also include information relative to wind speed or distance of the radiosonde from the launch station. This represents a useful information to address the possible co-location mismatch between radiosonde and lidar/ceilometer data and to better evaluate the time average solution used for comparing the BL retrieval algorithms using radiosonde and lidar/ceilometer observations.

12. page 577, line 9-13: this sentence is quite unclear “Also, rapidly-ïňĆuctuating SML and DRCL upper boundaries are 10 hard to detect using lidar and ceilometer especially when the DRCL is shallow and the instrument vertical resolution can not resolve properly the weak gradients”. Please explain better.

13. page 578, line 26-28: Please include a reference for supporting the aerosol-based boundary layer definition.

14. page 579, line 3-5: the authors should be very careful in providing any remark

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relative to the efficiency of the aerosol-based boundary layer definition using the results obtained from only one case study. This is definitely not sufficient. Moreover, this remark is relative to the detection of an inversion in the temperature profile at 315 m of altitude. Therefore, see next comment.

15. page 579, line 18: the authors are comparing an inversion detected in the radiosonde at 315 m with the lidar and ceilometer observations and they found no corresponding gradients in the lidar/ceilometer backscatter profiles. First of all, it is necessary to clarify that just one case does not make sense in a statistical analysis. However, this comparison is dubious. Your results could strongly depend on the lidar/ceilometer systems you are considering in this study. The disagreement could be related for example to the presence of a small amount of aerosol and to the low sensitivity of the lidar/ceilometer system to the small aerosol loading. Do you have any data relative to the sensitivity of these systems respect to different aerosol optical thickness? Moreover, the ALS 300 has a nominal full overlap height of 200 m, that is very close to the inversion height observed in the radiosonde temperature profile (315 m). Has the nominal overlap of the ALS300 system been never assessed before by the authors? Finally, according to the data reported in the plots, the case study the authors are considering for supposing the existence of possible limitations in using aerosol as a tracer for the retrieval of BL height is also the only case where the BL is located below 450 - 500 m of altitude. This seems to agree with the above mentioned sensitivity or overlap issues.

Technical Corrections:

1. in section 1. page 566, line 5, please change “”homogeneously” in “more homogeneously”

2. in section 1. page 566, line 12, please change “”up in the troposphere” with “in the upper troposphere/lower stratosphere region”

3. page 567, line 5-6, please modify the sentence as follows: “Previous intercompar-

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ison studies report the improved efficiency of ceilometers in the detection of the BL respect to lidar techniques”

4. page 568, line 1-2: I suggest to move the daily timetable of radiosounding launches when the radiosounding data are discussed or at least to repeat later in the manuscript to increase the readability.

5. page 568, line 6: the authors should briefly report the manuscript structure.

6. page 568, line 15: replace “sensor” with “station equipment”

7. page 570, line 4-6: is this sentence copied by the manual? I suggest to remove it, because it is not necessary for your description.

8. page 575, line 8-11: see previous comment.

9. page 575, line 17, 19 and 25: please, stay consistent with the data format.

10. page 576, line 17: see comment #19.

11. page 581, line 4: please replace “registered” with “showed” or another verb.

12. page 581, line 10: please correct CHM15k.

13. Fig. 3 and Fig. 4: please specify in the text and the caption what are the horizontal and vertical error bars.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 563, 2011.